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Pure line (Not included in the dictionary.) *A group of individuals derived solely by one or more self-fertilizations from a common homozygous ancestor. Sometimes erroneously applied to groups of individuals believed to be genotypically homogeneous (a homozygous biotype or a clone) without regard to their method of reproduction.*

Repulsion. (Not given a genetic definition in the dictionary.) *Such a relation between two genetic factors that both are not, as a rule, included in the same gamete, referring especially to cases in which the factors in question give rise to obviously different characteristics; also called ''spurious allelomorphism.''*

Sex-limited inheritance. (Not defined in the dictionary.) *The association of the determiner for any unit-character, with a sex-determiner, in such a manner that the two determiners are either generally included in the same gamete, or that they are generally included in different gametes. This method of inheritance is also called ''sex-linked'' inheritance by Professor T. H. Morgan and his students.*

Segregate. ''To become separated from the rest; specif., of Mendelian hybrids, to separate, by a numerical law, into dominants, hybrids and recessives.''

Segregate. *With reference to Mendelian unit-characters, to become separated through the independent distribution of the genetic factors before or at the time of the formation of the gametes.*

The dictionary definition goes too far; the formation of dominants, hybrids and recessives depends not alone upon the fact that the factors *segregate,* but that the segregated factors *recombine.* The word ''segregation'' receives a fairly satisfactory definition.

Unit-character. (Not included in the dictionary.) *In Mendelian inheritance a character or alternative difference of any kind, which is either present or absent, as a whole, in each individual, and which is capable of becoming associated in new combinations with other unit-characters.*

I have made no systematic study of the definitions of technical terms in other related fields, but have noted incidentally that there is no recognition in the New Standard Dictionary of the generally familiar usage of the words ''meristic'' and ''substantive'' as applied to types of variation.

G. H. Shull

PROFESSOR DE VRIES ON THE PROBABLE ORIGIN OF ŒNOTHERA LAMARCKIANA

In a recent paper Professor Hugo De Vries[1] has given us the results of a second examination in 1913 of material from the herbarium of Lamarck, and of other sheets of *Œnothera* in the collections of the Muséum d'Histoire Naturelle in Paris. This is a very important contribution for there have been some changes in the mounting of important specimens in the herbarium of Lamarck since the first studies by De Vries in 1895 and it was not clear what material formed the subject of his discussion in "Die Mutationstheorie." As the result of this second examination (1913) there can be no misunderstanding of De Vries's conclusion as to what represents the type of *Œnothera Lamarckiana* Seringe, and we have also very positive opinions on the identity of other interesting material in the collections at Paris. Thanks to his descriptions and photographs of these sheets further confusion will be impossible and botanists may now make for themselves the observations that will in the end determine their judgment of the soundness of Professor De Vries's views and of the value of the exceptions that may be taken to them.

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[1] De Vries, Hugo, ''The Probable Origin of *Œnothera Lamarckiana* Ser., *Bot. Gaz.*, Vol. LVII, p. 345, 1914.

I shall not at this time discuss in detail the queries which presented themselves on my reading of De Vries's paper. The most important of the points probably rest on facts that should be shown by the material, but which have not been published in the account of De Vries. I expected to have the data in question this autumn but the European disturbances have necessarily upset my plans and it may be very many months before I can take up the matter.

However, I will briefly say that De Vries's identification of the sheets under consideration are to me not convincing chiefly for the following reasons. His account gives no description of the pubescence of the sepals, stems, or capsules when present. Yet pubescence is a character of great importance in the description of many species of *Œnothera*. To illustrate the point, all races of *O. grandiflora* Solander that I know have sepals and capsules almost glabrous or very sparsely pilose and puberulent. *Lamarckiana* on the contrary presents sepals and capsules with a very heavy puberulent and pilose pubescence. Should any of the specimens at Paris which De Vries has identified with the *Lamarckiana* of his cultures present sepals or capsules lacking the heavy pubescence of this plant the fact to me would be very strong evidence that his identification was incorrect.

There are two sheets under consideration as standing for the type of *Œnothera Lamarckiana* Seringe. De Vries regards one as unequivocally representing the type specimen. I have for various reasons placed the greater emphasis upon the other. Both specimens as shown in photographs appear to have essentially the same features as to their general morphology. Miss

Eastwood and M. Gagnepain who compared the two specimens reported to me that they were very similar. Both were undoubtedly known to Lamarck since the two sheets bear his handwriting, and it is quite possible that Lamarck based his description on both specimens.

The general morphology of these specimens presents several features that are not those of the *Lamarckiana* of De Vries's cultures. Chief among these are (1) the approximate branches, (2) the foliage of narrower and more distinctly petioled leaves, (3) the inflorescence more open and with narrower bracts, (4) the buds more slender and tapering, and apparently with more attenuated sepal tips, (5) the long delicate hypanthium. In these features the specimens are closer to *O. grandiflora* than to *Lamarckiana.* Such morphological characters, it is true, might vary somewhat under different conditions of growth and with the time of collection whether early or late in the season. The pubescence should give us the stronger evidence of relationship since pubescence would be little if at all affected by growth conditions or by season. Of the pubescence on one of these specimens I have Gagnepain's statement that it is close to that of *grandiflora,* but it is only fair to say that no *Œnothera* specialist has reported upon such a comparison as is desired.

Lamarck's description of the capsules of his plant as short and glabrous is a point of great importance. The capsules of De Vries's *Lamarckiana* are certainly not glabrous but they are short. In my contention that Lamarck's plant was a form of *O. grandiflora* Solander I was at first forced to assume that Lamarck must have described immature or partially pollinated capsules. I have, however, this summer grown œnotheras from Mississippi which have the rosettes, habit, foliage, inflorescence, and flowers of *grandiflora,* but which developed glabrous short capsules essentially of the same relative proportions as those of *Lamarckiana.* It is immaterial what is the origin or genetic history of these plants; systematically speaking they represent short-capsuled forms of *O. grandiflora.* Thus we now know of *grandiflora*-like types which even as to their capsules agree with the description of Lamarck. De Vries does not seem to be disturbed by the fact that the material of his cultures presents capsules with a heavy puberulent and pilose pubescence while Lamarck's description specifies a capsule "glabre."

In summary I must say that my opinion remains unchanged with respect to the affinities of the plant described by Lamarck, *Œnothera Lamarckiana* Seringe. Whichever of the two specimens considered above represents the type, or if both were concerned in the description, the evidence is to me very strong that Lamarck dealt with forms of *O. grandiflora* Solander. I can see no proof or even reasonable evidence that the *Lamarckiana* of De Vries's cultures agrees with either of the specimens from Lamarck's herbarium. A final judgment, however, should not be made until we have before us details respecting the pubescence of the specimens known to Lamarck.

De Vries is very positive that two other sheets in the collections at Paris present specimens agreeing with his *Lamarckiana.* The first of these (De Vries, Plate XVIII) is from the herbarium of Abbé Pourret and shows material which seems to me to offer very much the same difficulties to an identification with De Vries's *Lamarckiana* as do the specimens of Lamarck. The foliage of lanceolate leaves clearly petioled, the slender tapering buds, the long delicate hypanthium; these are not characters representative of the plants from the cultures of De Vries. They are characters of *O. grandiflora* Solander and should the pubescence prove to be similar to this species I should not hesitate to place these specimens of Abbé Pourret among the forms of *grandiflora.* Until we know the facts of the pubescence, further discussion is unwise, but it does not seem to me that De Vries's identification rests on good evidence.

The remaining sheet at Paris which De Vries (Plate XIX) identifies with his *Lamarckiana* is a plant from the herbarium of André Michaux. De Vries on historic grounds naturally attaches importance to this sheet for if it could be established as in agreement with his plants the fact would bear directly on the problem of the origin of *O. Lamarckiana.* The flowers are large and the buds rather stout as we find them in our cultivated *Lamarckiana,* but the sepal tips are longer and the bracts much narrower than in *Lamarckiana.* The most striking characters of this specimen as shown in the photograph are the narrow lanceolate leaves and the extraordinary length of their petioles. That such a plant could be related to De Vries's *Lamarckiana* which has ovate-lanceolate leaves, sessile or almost sessile, seems to me well nigh impossible. Of the pubescence De Vries tells us nothing, yet the numerous buds on the specimen should make it easy to determine this character and it may become a crucial point in judging the possible or impossible relationships of the plant.

In the discussion which must develop from the conclusions of De Vries he has taken by far the more difficult position since he attempts an identification of herbarium material with a type very accurately known to us through widely cultivated living forms. My argument is presented primarily against his identifications. It is not in any degree necessary to my argument that I should assign the sheets under consideration to definite species. Whether this can be done for any of them time will tell and I must repeat that as evidence the character of the pubescence may prove of the greatest value. I am working on the hypothesis that the specimens of Lamarck and that of Abbé Pourret are forms of *O. grandiflora* Solander. As for the specimen of André Michaux, so many remarkable forms of *Œnothera* are coming into the experimental garden from the southern and western United States that I am quite unwilling to express at present even a guess as to its affinities.

De Vries has welcomed my suggestion that the source of the cultures of Carter and Company may have been not Texas, as they state, but England. This possibility seems to me to offer an important line of investigation of early British records and collections, but at present the suggestion appears to me nothing more than a working hypothesis, although well worthy of attention. Texas and the West have some wonderful large-flowered œnotheras and Carter and Company may have obtained from such sources a plant which later hybridizing with other forms produced the *Lamarckiana* of our present cultures. That there are American western species which will hybridize with European *biennis* and produce a synthetic *Lamarckiana* is I believe established by my present studies with *Œnothera franciscana* Bartlett.

In recent papers I have reported that first generation hybrids of *O. franciscana* pollinated by the Dutch *biennis* have the essential taxonomic characters of the small-flowered forms of *O. Lamarckiana*. They differ from *Lamarckiana* in relatively small plus or minus expressions of these characters. It was to be expected that large F_2 generations would give a wide range of variation or segregation of characters and that forms would appear much closer to *Lamarckiana* than the parent F_1 plants. This proved to be the case in F_2 cultures of last summer (1914) totaling about 1,600 plants. Among these I obtained a number of individuals which were so close to the large-flowered *Lamarckiana* that flowering shoots could scarcely be distinguished as to pubescence, foliage, inflorescence, buds, flowers, and capsules. The rosettes were also *Lamarckiana*-like. Only in habit was it somewhat difficult to match the symmetry of *Lamarckiana.* That further selection in later generations is likely still to further improve on the results of this synthesis seems altogether probable. These studies will shortly be described in full.

I am well aware that a synthesis of a *Lamarckiana*-like hybrid even should it throw in successive generations a series of marked variants (mutants) will not be considered by De Vries and his disciples as casting doubt on the validity of the "mutation" of *Lamarckiana.* They will say that in this case the hybrid took its mutating habit from one or both of the parents. Since in my cross one of the parents is the Dutch *biennis* which Stomps has shown can produce *nanella, semi-gigas* and *sulfurea* mutants, it will be claimed that any behavior of my hybrids similar to mutation will be due not to the mixing of diverse germ plasms, *i. e.,* to crossing, but will be merely a further expression of mutating habits inherent in the germ plasm of at least *biennis* if not also of *franciscana.*

This phase of the discussion may rest until we know the future behavior of my hybrids and the possibilities of the Dutch *biennis* as a form capable of mutation. It is to be expected that Stomps will carry out his very important studies on a scale that will virtually exhaust the mutative possibilities of this species. Such a study on a close-pollinated species of *Œnothera* so well known as the Dutch *biennis* will give, it seems to me, the safest data that has yet been published by students of mutation among the *Œnotheras.* It becomes a matter of great interest to know the range of variants that such a type can produce. Similar studies among some of the wild American species should also be made. The open-pollinated assemblage of forms to which *Lamarckiana* belongs must always be open to suspicion of hybridization more or less remote in time or distant in relationship. Only prolonged experiment can establish an open-pollinated *Œnothera* as free from the taint of crossing.

It is, I trust, clear that one may believe very strongly that *Œnothera Lamarckiana* is not safe material on which to base experiments designed to test the mutation theory and yet remain receptive to evidence that may come from other sources.

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October, 1914